

Stuart Freedman
Boris Kayser
Chairmen, APS Neutrino Study

Dear Stuart and Boris:

At the meeting of the Fermilab Physics Advisory Committee this week, they held a long and thoughtful discussion of the Fermilab Long Baseline Neutrino program and the NO ν A (NuMI Off-axis ν Appearance) proposal. Part of the input to this discussion was the report of a committee that we assembled to review neutrino initiatives worldwide in preparation for the PAC meeting. The PAC produces a carefully considered written report at the end of each of its meetings. Although that report will not be made public until next week, we already have a version that is final but for some detailed wording. Because the APS neutrino study will start its meeting Monday, I thought I would convey a summary of the PAC report and the laboratory's reaction to it. All of the detailed evaluations in this letter are from the draft PAC report.

We are just about to add the MINOS experiment to the operating neutrino program. NuMI commissioning is proceeding on schedule with first beam extraction expected in December 2004 and substantial neutrino production starting in the first quarter of 2005. The Booster has already demonstrated the ability to produce more than enough protons for both NuMI running and anti-proton production. The ongoing program of upgrades to the Booster complex will provide the capability of delivering some beam to Booster neutrino experiments while the NuMI beamline is running.

The report of the Fermilab Long Range Planning Committee presents a vision of a potential future neutrino program. This vision includes further oscillation measurements with the Booster, if MiniBooNE results lead in that direction, and a program of low energy neutrino cross section measurements. The vision also outlines a series of steps in a long-term oscillation program following MINOS. The steps in that evolving program are:

- an experiment designed to measure θ_{13} with a sensitivity to $\sin^2 2\theta_{13} \sim 0.01$ and to determine the mass hierarchy if θ_{13} is not too small;
- a Proton Driver that would enable the sensitivity to non-zero θ_{13} to be pushed to $\sin^2 2\theta_{13} \sim 0.005$, and enabling determination of the mass hierarchy and the search for CP violation;
- possible second detector on the second oscillation maximum; and
- a possible future neutrino factory.

In this approach, each step would be guided by the results of earlier steps. The PAC endorsed the vision of a such a step-wise campaign to discover non-zero θ_{13} , followed by more precise measurement of $\sin^2 2\theta_{13}$, determination of the mass hierarchy, and search for CP violation. We would be able to react to the physics information as it becomes available without delaying the progress unnecessarily.

The PAC discussed the Fermilab program in the context of approved and proposed experiments around the world, as summarized by the neutrino initiatives committee. The approved and funded T2K (Tokai to Kamiokande) experiment in Japan, after five running years in its Phase 1, is expected to have the capability to discover ν_e appearance for values of $\sin^2 2\theta_{13} > 0.018$ by about 2014 if the full intensity is obtained quickly after the turn-on. Because of its relatively short baseline (295 km), T2K will not be very sensitive to matter effects, and therefore will not determine the mass hierarchy on its own.

Future reactor oscillation experiments will aim at sensitivity to $\sin^2 2\theta_{13}$ comparable to T2K, and are likely to be limited by systematics after a few years of running. Limits on $\sin^2 2\theta_{13}$ from reactor experiments depend only on Δm^2 , while long-baseline experiments probe combinations of $\sin^2 \theta_{23} \times \sin^2 2\theta_{13}$, matter, and CP effects. Reactor measurements therefore would provide complementary information to long-baseline measurements.

The NOvA collaboration submitted a proposal to the Committee at the April, 2004 meeting. Additional written and presented materials were submitted for the June meeting to address questions raised by the PAC, to further quantify and refine the physics case, and to describe the ongoing R&D program. The collaboration also presented the preliminary design of an attractive alternative detector based on a totally active liquid scintillator design (TASD). Simulations of this option show an improvement in efficiency of almost a factor of two, and a cost per mass that is roughly double that of the sampling calorimeter. Better background rejection capability and improved energy resolution may give this option better overall sensitivity.

The PAC said that to establish a compelling physics case, NOvA must meet the following criteria:

- 1) Uniqueness. Does NOvA have a unique physics capability not achieved by any other experiments worldwide?
- 2) Competitiveness with T2K, the Japanese program discussed above. Can NOvA compete with T2K program within a similar time frame?
- 3) Competitiveness and/or complementarity with future reactor experiments. Can NOvA compete with their sensitivity or provide information not obtainable from the reactor experiments?
- 4) Capability for evolution with the future neutrino program. Would NOvA allow natural progression to CP violation studies with a future proton driver with the currently proposed detector at the same location?

In the near future, NOvA would be the only experiment in the world that could potentially determine the mass hierarchy for a range of the relevant parameters. Its performance would be competitive with T2K in other areas, namely the search for electron appearance for $\sin^2 2\theta_{13} \geq 0.01$ and precision measurements of $\sin^2 2\theta_{23}$ and Δm^2_{23} . NOvA's electron appearance signature, which would be

statistically limited, is complementary to the disappearance signature from the reactor experiments, which would be systematically limited and insensitive to matter effects and CP violation. Observing electron appearance would make the case for a proton driver even more compelling, and would possibly motivate a second detector. **The Committee found that the proposal can meet the above four criteria, if the experiment can be built in a timely manner.**

Following the construction of a proton driver, NOvA, equipped with a second off-axis detector, would reach its full capability. It would be able to determine the mass hierarchy for any value of δ down to $\sin^2 2\theta_{13} \geq 0.02$, which in turn will allow 3 sigma discovery of CP violation for a large range of δ . A combination with the data from T2K-II would extend the reach in CP violation to much smaller $\sin^2 2\theta_{13}$.

In the context of a coherent long-range neutrino program, the PAC found the case for NOvA compelling. The physics goals are to first measure θ_{13} , then to resolve the mass hierarchy and to discover CP violation in neutrino oscillations. This is an attractive approach, proceeding in incremental steps that allow for decisions based on outcomes at each stage of the program, taking into account new results from other experiments, as well as funding constraints.

The PAC strongly endorsed the physics case for the NOvA detector, and said that they would like to see NOvA proceed on a fast track that maximizes its physics impact. Although the planning is more advanced than other unapproved neutrino proposals, it is not yet as advanced as Fermilab requires for Stage I approval. It listed the steps needed to achieve that approval:

- Finalize the detector design.
- Complete the proposed R&D program.
- Update the proposal to reflect the complete science case.

The PAC strongly endorsed the proposed R&D plan and urged the Laboratory to provide adequate support for timely completion of this program.

I agree with the analysis and conclusions of the PAC and will accept its recommendations.

To reach full understanding of the neutrino sector will require several experiments of different types. The worldwide program should include accelerator-based oscillation experiments of different baselines, a reactor neutrino experiment, and one or more double beta decay experiments. The complication is that each major experiment is now in the \$50-\$150 million range, and that the present budgets for particle and nuclear physics cannot accommodate a minimal program of such experiments without some modest increase. The compelling nature of neutrino physics and the record of recent progress in the field justify such an increase. Multi-MW neutrino sources and megaton-scale detectors are even more expensive, and will require larger initiatives.

Given the importance of the physics, the cost of the facilities, and the limits on resources, the neutrino program must be planned with more coordination than in the past. Each experiment must demonstrate that it will provide significant additional capability to the world program. In addition, the planning of the neutrino physics program in the U.S. needs to be done across the traditional boundaries of NSF and DOE, nuclear and high energy physics

I look forward to the output of the APS neutrino study. If there is anything else that Fermilab can provide to help advance the process, please let me know.

Sincerely,

Michael Witherell